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Interactive comment on "Organic Carbon Characteristics in Ice-rich Permafrost in Alas and Yedoma Deposits, Central Yakutia, Siberia" *by* Torben Windirsch et al.

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The manuscript by Windirsch et al. "Organic Carbon Characteristics in Ice-rich Permafrost in Alas and Yedoma Deposits, Central Yakutia, Siberia" examined and compared the organic carbon storage and characteristics from two rare and deep cores using different methods (C%, soil texture, 14C age, ice content, etc..). Since such deep cores are very rare, this study is very important and gives us valuable inside information about the history of these deep deposits.

Thank you very much for this general statement of importance of our study.

The scientific question and the used methods are well established, however, there is still room for some improvement. Overall, the discussion section is rather weak and speculative, and the based conclusions too shaky. One reason, most of the used methods were nor fully incorporated in the discussion section. Some results as for example 14C inversions or magnetic susceptibility are not really discussed and explained.

We are grateful for the review and acknowledge the reviewer's comments, which really improve and strengthened the paper. As suggested by the reviewer we extended and clarified the discussion and strengthened the conclusion. Please see our comments below.

Nitrogen data not presented, even though obviously available.

They have not been shown as most values did not reach detection limit. This has now been referenced and implemented in the result sections 4.1 and 4.2. Moreover, the data presented in the manuscript are available on the PANGAEA repository.

Conclusion to short, missing main points as for example that the global estimate of SOC in Yedoma might be by far overestimated and possibly not so vulnerable due to rather low ice contents

Yedoma ice contents are low in this specific case, which is described in the conclusions. With one site we can not revise circumarctic estimates. Therefore, general Yedoma vulnerability is not the topic of this discussion. As the drained thermokarst basins are 9 m deeper there is a substantial potential for Yedoma for severe changes. Using this study to revise studies basing on a Yedoma wide study site set (Strauss et al 2017, Schirrmeister et al 2011) is not appropriate to our mind. But nevertheless, as we state we make this point in the discussion but this is not one of the main conclusions.

Also, recent publications suggest Yedoma being not extremely vulnerable. Missing older Russian Literature

We checked on international available and peer-reviewed Russian literature and added some more suitable literature. We avoid citing non-peer-reviewed literature and literature that is not available to an international scientific audience. We would be happy to include more literature. Please use the comment function to add specific literature. Besides this, we concluded that Yedoma is highly vulnerable to thaw induced landscape changes due to loss of excess ice. However, as stated by Kuhry et al, the carbon stored in Yedoma deposits might not be highly vulnerable. We now implemented Kuhry et al. (2020) to the paper which was not published at the first submission of our paper.

Below I list more specific points, which should be addressed before final publication. P.1 L. 15:"has not yet", actually parts of Yedoma are already in the active carbon cycle;

We changed this to "this carbon becomes available to the recent carbon cycle.", as some of it is still frozen and immobilized.

P1 L: 30"very carbon poor", please add number;

Thank you, changed accordingly: We added "(< 0.1 wt%)" to the description, which is the detection limit for measuring the total organic carbon content.

P2 L2 "provide"???, and why is it important;

We changed this to "Permafrost deposits present one of the largest terrestrial carbon reservoirs."

P2 L6, reference on co2 in the atmosphere not appropriate. Use actual data source;

We changed the reference to 868 Gt which was calculated using the CO2 concentration of 407 ppm, measured in 2018. This was calculated using the conversion formula given by Friedlingstein et al (2019). Global carbon budget using a factor of 2.124 for conversion between ppm CO2 and Gt carbon after Ballantyne et al (2012) for well-mixed atmospheric conditions. The text now reads: "The estimated amount of frozen and unfrozen carbon stored in the terrestrial permafrost region is 1330 to 1580 gigatons (Gt) (Hugelius et al., 2014;Schuur et al., 2015), which is up to 45% more than what is currently present in the atmosphere (∼ 864 Gt, based on 407 ppm CO2 measured in 2018) (Ballantyne et al., 2012; Friedlingstein et al., 2019)."

P2 L16 “vulnerable” recent paper by Kuhry et al. states that Yedoma is rather stable.;

Concerning landscape vulnerability: the general high ice content in Yedoma deposits makes them vulnerable to dramatic landscape changes caused by thaw under climate warming; Concerning C vulnerability: Kuhry et al (2020) does not state the opposite but concludes that thaw vulnerability does not mean a high vulnerability/lability of SOM. This sentence was changed as follows: “In the context of global climate change, such high ice content with pore ice and syngenetic ice wedges render Yedoma deposits highly susceptible to thaw induced landscape changes (Schirrmeister et al., 2013) and ground volume loss causing surface subsidence.”

P2 L20 Is this the case only for carbon stored within Yedoma deposits???;

Thank you, we changed “Yedoma deposits” to “permafrost”.

P2 L21 released how? I guess you mean as carbon dioxide.;

We specified this by adding “... in form of gases such as carbon dioxide or methane,...”

P2 L25, please add ref.;

We added “(Strauss et al., 2013)” to the corresponding sentence.

P2 L27 Explain why below 3m less understood?

This is due to the fact that only very few studies have been examining long permafrost cores. We clarified that in the text: “...especially below 3 m, are still poorly studied, as only few studies examining long Siberian permafrost cores have been conducted (Zimov et al., 2006;Strauss et al., 2013;Shmelev et al., 2017).”

P2 L29 missing space;

changed as suggested

P2 L32 “822 Gt” this is wrong, the number refers not only to Yedoma, but refers to combined permafrost SOC in soils (0–3 m), deltaic alluvium and Yedoma region sediments.

Thank you for notifying. We changed it to "permafrost region“.

P3 L5 Your ‘only’ 2 research questions are going under in the paragraph. Please make them stand out.

We made the research questions more visible by adjusting the paragraphs.

P3 L24 “in Siberia”, is there no data for MAAT at least for Central Yakutia?

Thank you for your suggestion. We changed this to “The contemporary mean annual air temperature in Central Yakutia (measured at Yakutsk Meteorological Station) is -9.7 °C.”

P3 L27, No more data or drilling campaigns since Soloviev, 1973?

There are actually few accessible studies on the thickness of the Yedoma in Central Yakutia. The most cited studies are indeed Soloviev, 1959 and 1973. We changed this statement and include both references as follows: “The Yedoma deposits in this region can be more than 30 m in thickness as was already shown by older Russian works (Soloviev, 1959, 1973).”

P4 L23 “approximately every 50cm? Why this distance? Why not using visual changes in the core for the increments?

The cores were indeed sub-sampled after visible stratigraphic changes. However, larger homogeneous sections were sampled approximately every 50 cm. We specified our subsampling approach as follows “Subsamples were equally distributed along the cores. According to visual changes, we covered all visible stratigraphic layers and we sampled at least every 50 cm in order to capture specific sediment properties.”

P6 L4 check paragraph spacing.

Thank you, we adjusted it.

P6 Since the main question is carbon, a chapter on how carbon estimates were calculated would be more then appropriate.

Thank you. The methods of our carbon budget calculation are explained in chapter 3.2.6. We renamed the chapter title to “Statistics and bootstrapping approach for carbon budget estimations” to make this clearer.

P7L18 “material composition???” soil texture.

Changed accordingly

P9 L6 please remove rather, enough with the given SD.

Changed accordingly

P9, when you compare the grain sizes, you change between median and mean, why?

We corrected this and now use the mean for all data.

P10 L1 I don’t fully understand the SOC calculations. How many samples were used for boot strapping? Please add a table comparing these two cores with SOC data, TN data, DBD data, to 3 m meter, to the different used units and to the total.

Thank you. We expanded the explanation on the bootstrapping approach. Also, we added a table to show SOC contents for the individual core units, based on the bootstrapping results. As TN data are not used in the bootstrapping approach, we did not include them in table 2 (please see below). These data are shown in the PANGAEA repository as most values are below the detection limit. Please see our comment further above.

P11L1-5. And what exactly are you arguing? Also, a lot of speculations. Please stick to the point. Also, the rather obvious reason for the low C comes first on page 13!

This was a misuse of “arguing”, we apologize. We changed the sentence into: “On the one hand, low TOC content could be a result of high organic matter decomposition during accumulation or during a thawed state, especially in thermokarst deposits. On the other hand, this could reflect low carbon inputs. The low stable carbon isotope data of our cores (between -24.06 and -27.24 ‰ are comparable to other studied sites from the Yedoma domain (Schirrmeister et al., 2013;Strauss et al., 2013;Jongejans et al., 2018), and suggest that the source signal was more or less constant with time.". The reason for low C is discussed in more detail later in chapter 5.2.

P11L13-15 But then the frozen sections should have the same C/N ratio, but they don't?

They do not have the same C/N ratio, as the Alas1 core was once thawed completely, as far as we conclude from the water isotope data. To highlight this we change this part as follows: "In comparison to the mean C/N ratio of 10 in YED1, the mean C/N ratio of 8 for Alas1 may indicate that the Alas deposits were only slightly more affected by decomposition due to a temporary thawed state. The development of a talik in Alas1 probably led to increased decomposition of these sediment sections and thus leading to a reduced C/N ratio. As the carbon was freeze-locked in the YED1 core and therefore was not decomposed since deposition, this similarity in mean C/N ratios indicates that the original carbon state for the deposits at both coring sites were similar, assuming the same carbon source for both deposits."

P11 L18 "decomposed" if, then rather pre-decomposed.

Changed accordingly

P11 L21-26 Ok, so what is the reason for inversions? Section by far too short. And no discussion on the other dates.

We restructured this paragraph and clarified. We also added further discussion in the paragraph that now reads as follows: "We found age inversions in both cores with similar age and depth (YED1 49,232 cal yr BP, 1998.5 cm bs; Alas1 42,865 cal yr BP, 1967.5 cm bs) (Fig. 4 and 6, Fig. S2). While cryoturbation might seem an obvious explanation, we suggest that this process did not play a major role here due to the long-term frozen state of YED1. Rather, we assume that the age inversions indicate a temporary shift in sediment input at approximately 35,000 cal yr BP causing some deposit reworking in the watershed and the incorporation of older material into younger sediments. In addition, the dating of bulk sediments very close to the maximum datable age of $\sim$ 50,000 yr BP may cause a high uncertainty in the absolute ages of sediment layers (Reimer et al., 2013). Therefore, the rather small age inversions (> 49.000 cal yr BP to 49,232 cal yr BP in YED1, and 45,870 cal yr BP to 42,865 cal yr BP in Alas1) could be a result of material mixture in dated bulk samples. The radiocarbon ages above this age inversion align well with a simulated sedimentation rate, as shown in figure S2.“

P11 L32 “indicate colder climate conditions” How, and what does it mean for the core? Any refs for that?

We deleted this sentence, as it was unnecessary information without important implications for the core.

P12 L2 “explained by climatic variations” Is this an assumption or is there data? Need refs for this statement.

We restructured the whole paragraph and added more detail on isotope data and d excess: “The stable isotope ratio values of ice wedges (mean δ18O of -30 ‰ mean δ2H of -224 ‰ reflect winter precipitation and fit well into the regional pattern for MIS 3 ice wedges in Central and Interior Yakutia (Popp et al., 2006;Opel et at., 2019) while the d excess shows a much elevated value (16 ‰ compared to the regional pattern (Popp et al., 2006;Opel et al., 2019). It should be noted that the d excess values from the central core parts correspond well to the regional values from Mamontova Gora, Tanda and Batagay (Opel et at., 2019), while the others fit to those of the host sediments and are potentially overprinted by exchange processes between wedge ice and pore ice (Meyer et al., 2010). Due to the low number of datapoints, no meaningful co-isotopic regression could be calculated. The stable isotope composition of pore ice shows a co-isotopic regression of δ2H = 6.61 δ18O - 18.0 (R^2 = 0.97, n = 23), which is typical for Yedoma intrasedimental ice (Wetterich et al., 2011;2014;2016). The isotope values plot well above the GMWL of the cold season (Papina et al., 2017) suggesting a substantial proportion of (early) winter precipitation (usually characterized by high d excess values) for the pore ice, which is also evident for some units of the Batagay megaslump (Opel et al., 2019). The decreasing upward trend of pore ice isotopic δ values might point to a general cooling in Central Yakutia during the covered time span of our study. However, as it is accompanied by an opposite increasing trend in d excess, these values may be overprinted by secondary freeze-thaw processes in the active layer and rather reflect the strength of these fractionation processes (Wetterich et al., 2014)." Moreover, we strengthened our stable water isotope expertise by inviting Thomas Opel to the author team.

P13 The final reason for the low C% should be mentioned earlier. Also, this page can be shortened.

Thank you for this comment. We agree and moved this interpretation into section 5.1. We also restructured and shortened this chapter.

P15 L15 & 23, repetitive sentence. Also, main conclusion, Yedoma SOC estimates likely too high.

We removed the repetition. Thanks for this comment but following Schneider von Deimling et al (2015) the SOC estimations for Yedoma could still be correct, giving the Yukechi site as a low-carbon example. Also, due to the sediment diversity within the YED1 and Alas1 cores, the Yukechi Alas landscape does not represent a typical Yedoma composition.

P15 L28. "high ice content.... vulnerable" actually the opposite. You showed very little ice content except one one ice wedge. Yedoma "had an estimated" ice wedge and lenses content of up to 90%. These two cores have far less. So these Yedoma deposits are not very vulnerable.

The high volumetric ice content in Yedoma deposits is mainly due to the presence of large ice wedges in the area (please see new figure S7 in the updated supplement), as in this case in our Yedoma core. We have chosen the Yedoma coring location in the centre of a polygon for not hitting an ice wedge. In this context, the Alas core was not included, as it represents already reworked Yedoma deposits in which the Pleistocene ice has largely melted. The pore ice content of the Central Yakutian Yedoma sediment deposits is comparatively smaller, but the sensitivity of the sediments to thawing processes is retained as for the ice wedges. This is also shown by the numerous large thermokarst landscape features in the region, but also by the currently observed very rapid thawing processes.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-470/bg-2019-470-AC2-supplement.pdf

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Discussion paper

Table 2 – SOC contents for the individual core units, based on the bootstrapping results; calculations were done for 1 m²; the measurement data used in the bootstrapping approach (bulk density, TOC density) are provided in the data sheet in the PANGAEA repository; * refers to samples with TOC content < 0.1 wt%; for organic carbon pool calculations, we assumed a TOC of 0.05 wt% for these samples; note: we excluded unit Y2 in the calculations.

Core	Depth [cm bs]	Number of samples used in bootstrapping	Mean dry bulk density [10³ kg/m³]	Mean TOC content [wt%]	Mean SOC content (bootstrapping results) [kg/m³]
YED1	0 – 300	7	1190	0.42	4.48 ± 1.43
	0 – 714 (unit Y1)	13	1090	0.59	8.31 ± 1.41
	1010 – 1927 (unit Y3)	18	1172	0.10	0.86 ± 0.32
	1927 – 2235 (unit Y4)	5	910	1.14	11.50 ± 1.36
	total core	36	1105	0.46	5.27 ± 1.42
Alas1	0 – 300	5	1257	0.51	6.93 ± 2.90
	0 – 349 (unit A1)	6	1214	0.44	5.00 ± 2.55
	349 – 925 (unit A2)	6	998	0.05*	0.50 ± 0
	925 – 1210 (unit A3)	4	1299	0.05*	0.66 ± 0.01
	1210 – 1980 (unit A4)	12	1377	0.83	11.03 ± 1.62
	total core	28	1250	0.47	6.07 ± 1.80

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Fig. 1. Table 2

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